STATE OF CALIFORNIA—HUMAN RELATIONS AGENCY
RONALD REAGAN, Governor

DEPARTMENT OF PUBLIC HEALTH
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October 31, 1972

William H. Wymer
Executive Secretary
Federal Working Group on
Pest Management
Room 17-92
5600 Fishers Lane
Rockville, Maryland 20852

Dear Mr. Wymer:

The deadline of "the close of business on November 1" does not permit me to write as tight a document as we both might like, but the following comments will suggest the nature and depth of my convictions but many of the Task Group recommendations, and the methods used to derive them, are ill-advised.

I. Apples - This Subgroup report rests, among other things, upon the assumption that not more than 3% of the parathion deposited on an apple thinner's skin is absorbed. The citation is a study by Durham and Wolfe, published in Bulletin of the World Health Organization in 1963. In looking up the reference, one finds that this was not a controlled study at all, but an extrapolation from another study with another purpose, full of conjectures, all of which, in the words of the authors themselves, "would tend to... underestimate the importance of dermal exposure".

It is difficult to understand why the apple subcommittee rested its case on so frail a reed when truly definitive work on the absorbability of parathion has been conducted in the last few years by Dr. Howard Maibach--as the Task Group has been informed through materials distributed by the secretariat, and through hearing a personal report by Dr. Maibach at the April 6 meeting in Berkeley. Maibach has found the following uptakes of parathion from different body sites:

Site	Percent of Parathion, Applied Topically, Absorbed (Average of Six Trials)
Forearm	8.6
Hand (palm)	11.8
Abdomen	18.5
Hand (back)	20.9
Elbow (inner)	28.4
Scalp	32.2
Ear (back)	34.0
Forehead	36.3
Ear (inner)	46.6
Armpit	63.9
Scrotum	101.0

Even if some of these sites are ruled out on the grounds that they are covered by farm workers' clothing, it is readily apparent that the absorbability of parathion deposits on the skin ranges between 8.6% (forearm) and 46.6% (ear). A weighted anatomical average-taking into account the amount of skin on the average person's head and neck, for example, compared with his arms and hands--would almost certainly not be less than 20%. This is approximately seven times the figure assumed by the apple subcommittee, on which all their subsequent figures rested. If the remainder of their method is to be retained (e.g., 8.1 milligrams of parathion per hour--81.5 milligrams per ten-hour day--deposited on the skin of apple thinners working in trees treated three days previously with parathion 25 WP), their recommended re-entry times need to be multiplied by a factor of seven, to provide an acceptable margin of safety based on the "best available scientific information". Maibach's work is the best available in its field.

A second key assumption of the apple Subgroup is that three days is an adequate waiting period for parathion in apples since few, if any, worker poisonings have been reported in the years that this period has been observed in Washington State. This, too, is a frail reed, and hardly the "best available scientific information". In California, which by common consent has the best reporting system in the country, less than one in two hundred fifty putative cases of occupational illness among farm workers, attributed to pesticides and other agricultural chemicals, come to the attention of official reporting mechanisms. (California State Department of Public Health, Community Studies on Pesticides, Contract Report #19, January 15, 1970, pp. 99-107). One might reasonably expect underreporting to be even more serious in states where farm laborers are not covered by Workmen's Compensation insurance, or are covered for job-connected injuries but not illnesses.

II. Citrus - The innovation of waiting plus washing seems to rest, at bottom, on two assumptions:

A. That washing citrus trees "with a dilute detergent solution or perhaps even with plain water (is) effective in markedly reducing dislodgable residues on fruits and...also on foliage". To the best of my knowledge, this proposition has been tested with only one of the compounds under consideration (i.e., an organophosphate Federally registered for use on citrus at the present time)--namely, dioxathion (Delnav). The pertinent results were as follows:

DAYS BETWEEN SPRAYING AND WASHING	FOLIAGE RESIDUES (UG/CM^2)			FRUIT RESIDUES (p.p.m.)		
	Before Washing	After Washing	Change	Before Washing	After Washing	Change
10	0.83	0.50	-37.5%	4.23	4.53	+ 7.1%
38	0.97	0.67	-30.0%	4.63	3.50	-23.9%
46	0.73	0.50	-28.6%	3.97	3.43	-15.0%

Note: Residues represent the average of three replicates.

Source: W.E. Westlake, F.A. Gunther, and G.E. Carman, "Worker Reentry Research: Dioxathion (Delnav) Residues on and in Orange Fruits and Leaves, in Dislodgable Particulate Matter, and in the Soil Beneath Sprayed Trees". Submitted for publication in Archives of Environmental Contamination and Toxicology, October, 1972.

The word "markedly", in the Subgroup's report, is, of course, a subjective one, but it is difficult to see how the foregoing data can be construed as indicating that washing is "markedly" effective in reducing dislodgable residues on either fruit or foliage. The remarkable thing, rather, would seem to be that so much dislodgable residue remains after washing--regardless of whether the washing takes place after 10, 38, or 46 days. As a matter of fact, other data from this same study show that virtually as much (and in some cases more) Delnav residue can be removed by simply shaking the trees as by washing them:

DAYS BETWEEN SPRAYING AND SHAKING	FOLIAGE RESIDUES (μG/CM^2)		
	Before	After	Change
13	1.07	0.60	-43.9
35	0.73	0.53	-27.3

Note: Residues represent the average of three replicates.

Source: Ibid.

It is perhaps even more instructive to translate the dislodgeable foliar residues from $\mu g/cm^2$ to ppm. The rule of thumb is that 1 $\mu g/cm^2$ in citrus foliage is equivalent to 70-80 ppm. In short, the "best available scientific information" is that about 40 ppm of Delnav are available after waiting 10 days and washing--which is the Subgroup's recommendation for the "safest possible practicable procedure". One knows of no evidence that levels this high, of a compound with Delnav's toxicity (43 mg/kg oral; 235 mg/kg dermal), can be reconciled with worker safety. On the contrary, one knows of a quantity of evidence that citrus residues a good deal lower than this, of Delnav and of compounds with roughly the same order of toxicity, are unsafe in terms of cholinesterase depression and/or clinical symptoms.

If the recommendation with regard to Delnav is thus invalid, it throws into grave question all the other recommendations for washing-waiting, too, since they were all based on extrapolation.

B. The citrus Subgroup's recommendations also rest heavily on the assumption there have been no useful "controlled experiments and clinical evidences of induced biological responses correlatable with... residues present at time of harvest". This assumption, also, is open to serious question. There have been more studies in citrus than in any other crop: controlled experiments involving two organophosphate compounds, alone and in combination; epidemiological investigations of at least six clinical poisoning incidents involving at least 43 different workers; not to mention an uncommonly large number of foliage residue studies independent of physiological effect. Although these studies were not ideal, in terms of the high standards the Task Group will presumably recommend in its final report on research protocols, they were good enough to permit fairly secure conclusions regarding safe re-entry periods. For example, applying the analytical method suggested by Serat, and distributed to Task Group members nearly six months ago ("Analysis of Pesticide Residues and Methods for Establishing Safe Re-Entry Times") it may readily be calculated, from residue and cholinesterase data also distributed to the Task Group many months ago, that if one wants to be sure of less than 30% mean cholinesterase inhibition from ethion in citrus, one needs to wait 24 days after application (assuming 7 pounds AIA of 25 WP; 5 days of work per week, 8 hours per day; etc.). Similarly firm conclusions may be drawn from studies of azinphosmethyl.

The most nearly "scientific" method, it would see, is to proceed from these conclusions -- putting them together with what is known about the comparative absorbability of compounds (from Maibach's work), comparative oral and dermal toxicities (known in almost every case), and comparative persistence (known in most cases, although not necessarily on citrus). This, in essence, is what the California Department of Agriculture's Advisory Committee on Worker Safety did in arriving at citrus re-entry periods which achieve adequate pest control, require no radical changes in agricultural practice, and, so far as we of Community Studies on Pesticides have been able to discern, are quite consonant with worker safety.

C. The lack of citrus picker illnesses reported in Florida, cited on page 3 of the Subgroup report, is irrelevant, for reasons mentioned in the discussion of apples, above.

III. Peaches - This Subgroup report, again, rests upon assumptions which are tenuous, to say the least.

A. The assumption is made that it is permissible to extrapolate from a study at peach thinning time, early in June, to peach picking time which takes place in August and September. This is a dangerous extrapolation, for a number of reasons.

1. Climatic conditions are different
2. The physiological condition of workers is probably different. Early in the season, cholinesterase reserves have not begun the slow process of depletion, which we have discussed many times in Task Group meetings, and which apparently reaches a point, late in the season, where an additional body burden that might have been borne with impunity earlier is sufficient to push a person "over the edge".

3. Exposure of pickers may be significantly heavier than that of thinners.

   a. There is not the accumulation of particulate matter early in the season that there is later.

   b. A thinner, by definition, need not reach every fruit; a picker, at stripping time,does.

   c. Depending to some extent on whether thinning is early or late, there is probably some foliar growth between thinning and harvesting. Larger leaves = more leaf contact.

4. The amount of active ingredient per acre in the thinning study (1-2 lbs.) was consistent with the Agricultural Extension Service's recommended schedule, but anyone who really wants to may legally apply 2.5 pounds AIA.

5. The 1971 peach thinner study involved the first application of the season--understandably enough, in order to ensure that the orchard was free of other materials. However, the most important finding in the peach picker study by Milby, et al., in 1963, was that "illnesses were the result of residue accumulation related to total amount of parathion applied during the entire growing season," rather than any single application. It is surprising that this cardinal implication of the Milby study was lost on a Task Group which was chaired by Milby himself.

B. As if extrapolation from peach-thinning to peach-picking for parathion were not, in itself, more than tenuous enough, the tenuousness is compounded by extrapolating from parathion to all other OPs, at both thinning and harvest times. If this sort of thing is done (and a certain amount of it may be necessary), it should certainly incorporate the principle just referred to: that buildups throughout the season call for longer re-entry periods close to harvest than earlier in the season. One would suppose this might be particularly true of compounds which are under no EPA limitation on the total amount which may be used cumulatively throughout the season.

One of the aftermaths of the Milby study was a Federal limit of 5 pounds actual parathion per acre between January 1 and harvest, but such materials as carbophenothion (Trithion) are nearly as toxic and at least as persistent as parathion, are recommended by the Extension Service for all phases of peach pest control from early spring to postharvest, yet contain no such limitation.

C. Some of the Subgroup's assumptions about toxicity and persistence seem unnecessarily crude. For example, mevinphos (Phosdrin) is assigned both a dermal and oral toxicity index of 4, which is less than that assigned to ethyl parathion and demeton (Systox), and the same as that assgned to Guthion, ethion, and others. The evidence from white male rats, which would seem a more valid basis for comparison than inter-species or inter-sex differences, suggests that Phosdrin is actually more toxic, both orally and dermally, than either Systox or parathion, and of a wholly different order of magnitude than ethion or Guthion (over 50 times as toxic, dermally, as ethion, e.g.).

When the arbitrary (and sometimes inaccurate) toxicity indices are multiplied by the arbitrary (and sometimes inaccurate) persistence indices--which the Subgroup did, rather than simply adding the two--the arbitrariness (and, sometimes, the inaccuracies) are compounded. The effect, for example, is to make Phosdrin appear virtually as innocuous as malathion (a "hazard index" of only 4 for Phosdrin, 3 for malathion), while parathion appears no more hazardous than Zolone, and less hazardous than the likes of Guthion and Delnav.

IV. Grapes - For reasons mentioned above, extrapolation from a study of peach thinners, involving a single application of a single compound, to all compounds, at harvesting as well as thinning time, stretches the word "scientific" beyond any customary meaning. What, then, can be said of another quantum leap, from that same peach thinner study to all compounds in a totally unrelated crop~~,~~--grapes,--and to all the manifold activities in that extraordinary crop: suckering, lateraling, girdling, bunching, leafing, harvesting of wine, table, and raisin varieties?

The thing which makes this leap particularly breath-taking is that it is so unnecessary . More solid data exist for grapes than for any other crop, with the exception of citrus. American Cynamid did an admirable residue study of dimethoate on grape foliage; a controlled experiment was conducted, under the aegis of the University of California, of the breakdown rates of four compounds, and the cholinesterase levels of pickers.

It is true that the nature of the latter experiment was such that one cannot say, for a certainty, which compound was the major cause of the cholinesterase depression observed. But it is not true to say that no conclusions are possible about the respective degradation of the materials. It is known, from an earlier study in citrus (which was distributed to Task Group members) that "the rate of Guthion WP decay is unaffected by the presence of ethion WP".

These two compounds, in wettable powder form, were the first two applied in the grape picker study.

It is also known, from the internal consistency of the curves in the grape study, that the decay of Guthion and ethion were not affected by the addition of either Zolone or Dibrom--and that the Zolone was not affected by the Dibrom. This conclusion was explicitly stated by the chemist to whom the directors of the grape study turned their residue data for interpretation; was distributed to all members of the Task Group. (See W.F. Serat, *op. cit.*, pp. 11-12). By means of a standard formula, this analyst constructed a table from which it was possible to tell, at a glance, the time periods required to reach desired residue levels of various compounds applied at various dosages. For example, Zolone applied at the Extension Service's recommended rate of 2 pounds AIA would take 38 days to degrade to 25 ppm on grape leaves; Guthion applied at the recommended rate of 0.75 pounds AIA, would take 33 days to degrade to 25 ppm; etc.

One might hesitate about extrapolating these findings to other compounds, and to other grape activities besides picking. But it seems self-evident, whatever their shortcomings might be, that such extrapolations would be infinitely less fanciful than the procedure of resting grape re-entry periods on a study in an utterly dissimilar crop.

V. Tobacco - Since the Task Group never heard an oral report from this subcommittee, it is hard to comment on their methods. However, one notes that, with regard to the six compounds where a comparison is possible, the Subgroup recommends one re-entry period which is the same as the existing EPA pre-harvest interval, and five which are shorter: 4 rather than 15 days for methyl parathion, for example.

This brings up a question which is implicit in all five Subgroup reports. I continue to find great difficulty understanding why the Task Group was created in the first place if, as these recommendations imply, farm laborers are more than amply protected by existing pre-harvest intervals. I have little difficulty, though, envisaging the reaction if the Task Group issues recommendations which, in effect, would strip from farm workers much of the protection they enjoy, albeit inadvertently, from existing pre-harvest intervals.

It is most difficult to conceive that the exposure of even the heaviest smoker of cigarettes, cigars, or pipe tobacco, or dipper of snuff, or baseball pitcher who chews tobacco, could remotely compare with that of the flue-cured tobacco worker who practically swims in whatever residues the leaves may contain. It appears to defy common reason to say that the consumer should be protected by a 15 day re-entry period, and the worker only 4 days. Most of the other Subgroup proposals (nearly 80% of them, by my calculation) are similarly defiant of common reason.

It would seem, as a practical matter, that there can be only two consequences of such apparently contradictory recommendations. (1) There will be a hue and cry from one side that EPA pre-harvest intervals are excessively long; there will be pressure to revise them downward, drastically in some cases. (2) There will be an equally fervent outcry from the other side that the proposed "worker re-entry periods" are excessively short; that they reduce an already perilously narrow margin of safety for harvest laborers; that they cannot be supported by any experimental evidence (almost all controlled studies to date have found that PHI's are too short, not too long, for harvest worker safety); that they expose producers to the likelihood of widespread crop condemnations, in the confusion between the two sets of intervals which is certain to follow.

It is not difficult to imagine a veritable Götterdämerung, in which, at one stroke, not only the concept of worker safety re-entry periods but the whole concept of pre-harvest intervals for the purpose of crop tolerances crumbles into disrepute and chaos. This, one ventures to suggest, was not the purpose for which the Task Group on Occupational Exposure to Pesticides was appointed.

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Although it is painfully late in the day, it may not be too late to return to the original legitimate purpose for which the Task Group was created. If adequate information for setting re-entry periods does not exist, the Task Group should say so--and should press on with the business of defining "adequate information" and recommending how it might best be gathered. If the Task Group permits itself to be put in a position of setting re-entry periods in the absence of information, it has betrayed its mission and practically guaranteed that the necessary research will never be conducted. For if, by its own actions, the group concedes that solid evidence is really not an essential precondition of re-entry periods, why should anyone thereafter go to the time and expense and travail of designing and executing a complex piece of research? "If they can set periods sitting in an armchair, so can we," will inevitably be the watchword thenceforward.

How did the Task Group arrive at such an extremity? Apparently, through (1) a petition to the Occupational Safety and Health Administration, U.S. Department of Labor, from certain migrant legal aid groups, calling for "temporary emergency safety time intervals," among other things. (2) a statement by representatives of the Department of Labor that if the Task Group did not set these "interim standards," they would be set, anyway, by DOL personnel; (3) the Task Group's fear that re-entry periods set without its "input" would of necessity be ill-conceived and "unscientific".

Each of those considerations warrants re-examination. To take them in reverse order: (3) It is gratuitously insulting to Dr. O'Neill, and others in OSHA, to suggest that they were incapable of such simple manipulations as (for example) multiplying 4 times 4 to get a "hazard index" of 16, and, by comparing this with a constant ratio of 15:12, arriving at a re-entry interval of 14 days. It is, by the same token, gratuitously arrogant to suppose that just because such manipulations pass through our hands, they somehow become sanctified and "scientific". Nothing can transmute dross to gold. It is not the proposals which have become more "scientific," but the Task Group which has become less.

(2) In other words, even if the Department of Labor made good its "threat" and promulgated interim re-entry periods on its own, they could scarcely be more rash than those of the Task Group--and, with all due respect, they might even be an improvement in some ways. Whichever the case, such a procedure would have very real advantages over the present one. Regulations issued entirely on DOL's own recognizance would probably be easier to change than those bowed down by the weight of a prestigeful "scientific" body drawn from the far corners of the nation. (We can all agree, I trust, that interim re-entry periods are going to need to be changed, probably sooner than later). Secondly, if the Task Group keeps a seemly distance from the interim-period imbroglio, it stands to gain the breathing space necessary to complete the assignment with which it began: i.e., a report concerning standard research protocols. Otherwise, there will likely be no such breathing room. Everyone who played any part in the formation of interim periods stands to become hopelessly bogged down in the litigation and political brawling which are virtually certain to follow.

One Task Group member said, at the last meeting, that "once these things go to the Federal Register, the onus is entirely on Labor". Such optimism is touching, but quite unrealistic. Any litigant is going to insist on putting on the witness stand the persons whose advice lay behind the DOL regulations. Even if DOL wished to protect the identity of the Task Group, and its Subgroups, it could not do so, for these are not matters affecting the national security and cannot be stamped "top secret".

(1) The greatest pity of all, in the debacle which seems to be gathering, is that it is so unnecessary. It rests upon a needless misreading of the intentions of the farm labor action groups whose petition of September 1, 1972, initiated the whole train of events. This petition was a "fishing expedition" just to see what they could get, if anything. They would have settled for a response from the Occupational Safety and Health Administration in terms of field sanitation, equipment safety, reporting, designation of agriculture as a "top priority industry", and/or appointment of bonafide farm workers to the Agricultural Advisory Committee--with re-entry periods deferred until they can be based on truly adequate, controlled research, and can be promulgated in a permanent rather than "emergency" form.

Assuming interim re-entry periods are promulgated at all, the citizen groups would consider it responsive to the petition if they were promulgated only for those few crop-compound couplings where something approaching a serious longitudinal study of controlled dosage and controlled human response has taken place: ethion and oranges; azinphosmethyl and oranges; ethion, azinphosmethyl and phosalone in grapes; possibly one or two others. Anything more is over-reactive.

Human organizations, once set in motion, gain a certain inertia, and it is often assumed, by those caught up in the institutional momentum, that this is the way things have to be. But that is not true of organizations generally, and it is not true of this Task Group specifically (nor of NIOSH, OSHA, or any of the other institutional actors in the present drama). No overwhelming cosmic force fates us to issue re-entry periods for all OPs on five crops, or even on one crop. Our lives, fortunes and sacred honor do not depend on it. No farm worker lives depend on it. Nothing of value depends on it. DOL may be under the illusion that issuing interim re-entry periods will get the petitioners "off its back", but nothing could be further from the truth. Re-entry periods which decrease (or, at least, give the appearance of decreasing) the protections which farm workers already receive, at second-hand, from EPA pre-harvest intervals, are a gilt-edged guarantee that the petitioners, far from being placated, will be outraged and infuriated, and will never rest until they have dragged before courts of justice and the court of public opinion all the parties they consider responsible.

There is a way out, and it is quite simple, really. We (or the Federal Working Group, or CEQ, or NIOSH, or OSHA, or anyone else in the chain of command) can say: "No. It would be contrary to the purposes of the petition--contrary to sound occupational safety--if re-entry periods were set for crops and compounds on which we have shaky information or none at all. We will issue regulations as promptly as the results of well-designed studies, along the lines of those recommended by the Milby Task Group, become available".

This response would not be challenged. But even if it were, it would be unassailable in a court either of law or of popular opinion. Can the same be said for re-entry periods derived by nothing more substantial than extrapolations twice and thrice compounded?

For all the aforestated reasons, therefore, I urge that OSHA respond to other sections of the September 1 petition, if it cares to, but that so-called "emergency" re-entry periods not be issued. If, despite the fact there is no "emergency", and despite the indications interim periods are likely to do far more harm than good to employers and employees alike--if, despite everything, OSHA is bound and determined to issue such regulations, I urge that the Task Group reconsider its participation in the process, withdraw whatever draft recommendations may now be circulating, and state the simple, dignified truth that there are not defensible grounds for setting such periods at this time (with the possible exception of the crop-compound combinations, cited above, in which there has been controlled prospective experimentation; these can almost literally be numbered on the fingers of one hand).

If these urgings are rejected, either because it is deemed too late in the day to turn back, or because the Task Group disagrees with me philosophically, I request a clarification in the Special Report now in preparation. The introductory section of the draft of this report states that "Subtask Group reports were read, discussed and accepted at (a) plenary session (of the Task Group)".

A listing of persons "who participated in the preparation of this Special Report" follows. It might be inferred, from this, that all participants "accepted" the Subtask Group recommendations. My participation was, in fact, limited to the expression of basic reservations, as the stenographic transcript of the October 18 meeting will show. If the Task Group proceeds along its present course, I would like this possible misunderstanding cleared up in one of two ways. Either drop my name from the list of participants (a privilege already extended to Jon May, Dick Ronk, Bob Spear, Dick Doutt, and Jake McKenzie); or make it very clear that there was a division of opinion among participants. I dissented then, and I dissent now, from most of the specific recommendations which were made, and, more fundamentally, from making any recommendations at all under existing circumstances. You may consider this letter as a Minority Report, if you wish, even though it be a minority of one.

Respectfully yours,

Henry P. Anderson
Research Specialist III
Community Studies on Pesticides

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